On the Combustion of Different Kinds of Fuel

I HAVE read with interest the criticism of Mr. I. Lowthian Bell (NATURE, vol. xix. p. 175) on my paper on the mode of combustion in the blast furnace hearth. You say with truth that the question is not simply technical, but is one of scientific importance. The prevailing opinions, which Mr. Bell has expressed with his usual force, rest upon experimental determinations of the gases in the hearth. I have never felt that trustworthy results have been obtained in any of the published analyses, and with your permission I would like to state the case, and see if my difficulties are removable by the wide experience of Mr. Bell or other investigators.

The blast-furnace hearth is a cylinder, closed at the bottom, but perforated near the top by a number of openings in which the tuyeres, or ends of the air blast-pipes are closely fitted. The air enters at a pressure which usually varies in anthracite practice between four and seven pounds to the square inch. As the discharge is at the top of the furnace, many feet higher, the air must describe a curved path from the point of entrance to the centre of the furnace, being acted upon continuously by a horizontal and a vertical force. It is evident that the level of the tuyeres is not the place to obtain the first products of combustion unless they are drawn through a tuyere in action. Elsewhere the samples would not be taken from the path of the air, which is upward from the tuyere from the instant it enters the hearth.

Mr. Bell and other investigators have analysed gases drawn from the hearth by means of porcelain tubes introduced through a closed tuyere aperture, or through holes drilled between the tuyeres. It seems to me these analyses are vitiated by the mode of drawing off the gas, and since this criticism applies to the experimental basis of existing views of combustion in confined spaces and with limited supplies of air, I will give a few details to show the scope of my objection.

At the Wear furnace Mr. Bell drew off gas through a tuyere that was closed for the purpose, but air was entering at other tuyeres on each side and four feet distant. Certainly this did not represent the product of that active combustion which takes place in the path of the air, but of these products after they had filtered through nearly four feet of glowing fuel. What the exact distance was depends upon the velocity of gas in the crucible of the Wear furnace and the inner diameter of the hearth, but was probably over three feet.

The quantity drawn off is not mentioned, but as it was taken for eudiometric analysis, the amount was probably less than five litres, and the movement of the gas through this glowing coal to the sample tube must have been extremely slow. Under these circumstances, whatever the product of combustion in the path of the air may have been, there could be only one gas drawn into the sample tube, and that would be carbonic oxide mixed with nitrogen. Even if we assume that the product of combustion in the furnace is carbonic anhydride alone (which is not true), this would be completely reduced to carbonic oxide by passing through the hot coal.

The experiments made on gas which was drawn through tubes in holes inserted between the tuyeres do not impress me more favourably. There the tubes were thrust "a little way into the contents of the furnace." That description does not apply to the mode in which gas samples were drawn off for analysis, but to experiments for testing the reducing powers of the gas by submitting pieces of ore to its action. Still it is probably also the mode in which samples were obtained, and the object of this note is to ascertain whether more careful means of sampling the unchanged products of combustion were used. If not, I submit that the analyses which form the basis of all modern reasoning on this subject must be rejected. The rapidity with which red hot carbon reduces carbonic anhydride, and produc s just the gas which experimenters find in their sample tubes, is well known, and methods of sampling which take no precautions to guard against this change cannot be accepted. I think the investigators owe it to science to give the world some hint of the means they have used to prevent this action, and to obtain the gas as it is formed. In the case of one of our American furnaces—a small on —the blast has an upward velocity of twenty feet per second at the level of the tuyeres, without considering the increase of volume by its rise in temperature in the furnace, and also allowing it to penetrate *instantly* to the centre, so as to cover the whole ar a of the crucible. That cannot be true, and on the other hand the withdrawal of gas from the walls at the tuyere level, while the air is entering with great velocity four feet away, can hardly give a fair sample of the unaltered result of immediate combustion.

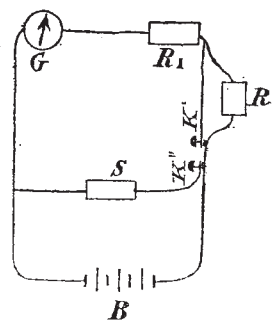
JOHN A. CHURCH

Columbus, Ohio, U.S.A., January 21

Internal Resistance

THE following method of measuring the internal resistance of a battery was devised some two years ago by Lieut. A. R. Conden, United States Navy, then attached to this station as Instructor in Electricity. It fulfils quite closely the conditions indicated by Clerk Maxwell on p. 412, vol. i. of his treatise on Electricity and Magnetism. As it is not generally known, I venture to call your attention to it.

In the figure, B is the battery, G a galvanometer, R and R_1 resistances, K" a key for introducing the shunt S, and K' another key for shunting out the resistance R.



When both keys are open the current through the galvanometer is—

$$S_1 = \frac{E}{B + G + R + R_1}.$$

When both keys are closed the current from the battery divides, part going through G and R_1, part through S. The current through the galvanometer is now—

$$S_2 = \frac{E}{B + \frac{(G + R_1)S}{G + R_1 + S}} \times \frac{S}{G + R_1 + S}.$$

If $S_1 = S_2$, then—

$$\frac{E}{B + G + R + R_1} = \frac{E}{B + \frac{(G + R_1)S}{G + R_1 + S}} \times \frac{S}{G + R_1 + S}.$$

Solving for B—

$$B = S\frac{R}{G + R_1}.$$

Finally, if R have been adjusted equal to $G + R_1$, then—

$$B = S \text{ directly.}$$

In practice R is a rheostat unplugged to equal $G + R_1$, and the two keys are combined in one. If the current be small enough with $G + R$ then R_1 may be omitted and R made equal to G. S is adjusted until, upon closing the double key, the deflection of the needle remains unchanged. The resistance of the battery is then the resistance of the shunt.

The case and concordance of the results obtained in this way through long series of measurements are no less striking than the rapidity with which the observations may be made.

Torpedo Station, Newport, R.I., C. F. GOODRICH

January 12

The Formation of Mountains

MR. G. DARWIN has shown that, on the supposition that the earth is a cooling solid, the depth at which the maximum cooling and consequently, in all probability, the maximum contraction takes place, moves downwards, and, taking Sir W. Thomson's values of the constants, has not yet got down so far as 100 miles.

This shallowness of the layer hitherto chiefly affected is alone sufficient to prove how small an effect can be attributed to such a cause.

He inquires whether I may not have under-estimated the contraction of rock in cooling. In my calculation I put it at ·000007 linear for one degree Fahr. I derived this estimate from

the consideration of Mr. Mallet's experiments on cooling slag run from an iron furnace.[1]

This coefficient is somewhat larger than the mean of those obtained by Mr. Adie[2] for much lower temperatures. The mean of six of his values, half of them being for moist rock and half for dry, I find to be ·0000057.

Mr. Darwin recalls attention to M. Favre's experiments (out of which the present correspondence arose). M. Favre's experiments illustrate well the structure of an alpine district. But I would observe that, if ours is a cooling solid globe, and if that would give rise to such surface structure, we ought to find it everywhere, and not confined to definite geographical areas, as we do. O. FISHER

Harlton, Cambridge, February 8

Concerning the Colour of Eyes

MAY a portrait painter be allowed to remark that there are two kinds of green eyes, and the poets have duly appreciated both. The eye of the "green-eyed monster" is, no doubt, the cold grey, or stony blue eye, overspread with the yellow of biliousness, hence green; but when Dante called the eyes of the beatified Beatrice *emeralds* he did not mean to insult her. The image called up by his ecstatic words is that of those deep, soft eyes which are a warm brown in some lights—for instance with the light falling on them from one side only—and take a grey tint when facing the light of the sky, and green tints at other times, according to the lights that fall upon them; and are therefore sometimes a puzzle to portrait painters. Eyes, like the sea and precious stones, catch lights and transmute them. The sea is only green from the meeting of sunlight and blue sky light in it. J. M. H.

P.S.—Has it been remarked that the distinction between yellow and blue tints—the only one made by the colour-blind, according to Dr. Pole—is precisely the same as that made by the sun in photography: all the warm tints (as an artist—who makes the same distinction—would call those partaking of yellow) coming out darker, and all the cold ones—or those partaking of blue—lighter than in the object photographed?

Intellect in Brutes

A CORRESPONDENT in NATURE, vol. xix. p. 268, describes the actions of a water-rat which, he says, climbed up to a window-sill, inconvenient of access, and thirteen feet from the ground, in order to get some bread which was habitually put there for the birds during the cold weather. As the rat had never found food there before, the writer concludes that his conduct cannot be attributed either to instinct or to experience, but must be ascribed to a process of reasoning based on the observation of the flocking together of the birds, and the inference that they must be attracted by food. Now it seems to me that before we ascribe to a rat such complicated reasoning powers it is necessary to ask if there is no other, simpler, way of accounting for the phenomenon. I think there is. It is well known that different species of animals vary greatly in the acuteness of their senses. To man, sight is the most important sense, and the same is true of many other animals, and most birds. The cat is a representative of another, smaller, class of animals, whose most perfect organ of sense is the ear; while the dog lives in a world of sensations, the most important of which are contributed by the sense of smell. To this last class bel ngs the rat, which is noted for the acuteness of its scent. It is evident, therefore, that the water-rat in question was led to the window-sill by his nose, which, in his case, was a more trustworthy guide than his eyes would have been. I do not wish to deny, by any means, that animals have reasoning powers. On the contrary, I am convinced that human and brute intellect differ only in degree, not in kind; and I even adopt Haeckel's "cellular psychology," which attributes the elements of intellectual life—sensation and volition—to infusoria and organic cells in general, in opposition to the older "neural psychology," according to which psychic activity begins with the nervous system in the scale of animal life. But what we have to guard against is not to ascribe to animals reasoning powers of a higher type than is consistent with the development of their brain, especially when the actions which seem to postulate such powers can be readily accounted for by simply bearing in mind the extraordinary acuteness of one or more of their senses. We are altogether too prone to judge the intellectual life of animals by the human standard, to imagine that the eye is everywhere, as with us, the leading source of knowledge; and the neglect of the important *rôle* which the sense of smell plays in animal life has been particularly fruitful of errors in philosophical speculation. It has, among other things, helped to give a longer base of life to the old theory of instinct, regarded as a mysterious power of nature.

Berlin, February 8 HENRY T. FINCK

[1] *Trans.* Roy. Soc., paper read June 20, 1872.

[2] *Trans.* Roy. Soc. Edin., vol. xiii. p. 370.

Ear Affection

THE remarkable phenomenon described by your correspondent "P," in NATURE, vol. xix. p. 315, induces me to bring to your notice that precisely the same effect was produced in my own case a month ago, when partial deafness came on in both my ears, whilst suffering from congestion of the mucous membrane of the nasal passage and eustachian tube. Not being aware that any prior case had occurred of a distinct difference of a semitone, as indicated by the alternate application of a tuning-fork to the two ears, I at once drew up a memorandum on the subject, and handed it to Dr. Urban Pritchard, who was advising me. Like your correspondent "P," I have also noticed the double sound produced when I whistle, and more particularly when I close both ears with my fingers. G. L. WALLICH

February 11

Bees' Stings

THE American *Quarterly Microscopical Journal*, published last October in New York, contains an elaborate article on "The Sting of the Honey Bee," by J. D. Hyatt. Mr. Hyatt's experience does not tally with that of your correspondent, R. A. He says: "By allowing the bee to sting a soft piece of leather an excellent opportunity is offered for studying the action and mechanism, for the whole apparatus will be beautifully dissected, the *bee not appearing to be seriously injured by the loss.*" I should be happy to send the journal to R. A. if I knew his address. W. RADFORD

Sidmouth

Electric Lighting

I NOTICE in an article in NATURE, vol. xix. p. 262, the following reference made to our electric light that it "does not appear to give very great satisfaction through its fluctuation." It is true that at first we were caused some trouble owing to the Serrin lamp not working properly, but having overcome the difficulty we find it in our business, where it is necessary to show colours correctly, a very great improvement on all our former trials of lighting, and moreover, in its use we are not troubled in our galleries and upper floors with the heat and fumes which with gas alight no amount of ventilation seemed to remove.

It is not a pleasant light to read or write by owing to a certain flicker which seems common to all the regulators, but in warehouse or show-room use this does not cause any inconvenience, and we think in large places, especially those already having motive power, that it must eventually supersede gas.

Regent Street H. J. NICOLL

RELATION OF METEORITES TO COMETS[1]

II.

THERE are two classes of shooting stars which have been sometimes spoken of as unlike, but which are now admitted on all hands to be of common origin and character, namely, those which come in quantities on certain nights of the year, and give what is called a star shower, and the sporadic meteors, such as we can see on any clear night.

In November, 1799, von Humboldt saw during his travels in South America, a shower of shooting stars, and he has given a glowing description of the sight. These came on the morning of November 12. In 1832, November 13, there was seen in Europe a display of less brilliancy. It, however, attracted not a little atten-

[1] A lecture delivered in the Mechanics' Course at the Sheffield Scientific School of Yale College, U.S., by Prof. H. A. Newton. Continued from p. 317.